The authors state that intraoperative mapping would be useful in determining the indications for simplified procedures for AF. Only 8 patients had PVI, although 21 had passive RA activation. This indicates that further refinement in technology is required before intraoperative mapping could really indicate the choice of procedure.

The success rates for AF between the 2 procedures are probably similar because the authors have been selective in the choice of patients for PVI. It will be interesting to speculate what the results would have been if these 13 patients also had simple LA procedures. Intraoperative mapping confirmed that most activation arose from the posterior LA, the appendage, and the PVs and justifies the use of LA procedures for such patients. It is useful to know the exact sites of activation in those with predominantly LA activation. Perhaps with further refinement in mapping techniques, it would be possible to develop abbreviated lesion sets even shorter than the ones that currently exist. For instance, it might not be necessary to ablate all PVs if only specific veins are involved.

The finding of activation from the LA appendage justifies appendage exclusion. The effect of this on appendage-sparing procedures needs to be analyzed. Did the authors ablate the appendage, even if it was not the focus of activation? If so, why?

Patients in the study with LA diameters exceeding 60 mm but with viable electrical activity were cured of AF. This suggests that (1) the presence of viable electrical activity is a greater predictor for AF conversion rather than LA diameter and (2) mapping is useful in predicting the probability of AF conversion in these patients.

The mapping time is acceptable, provided mapping reliably indicates the choice of procedure, and will reduce with the advent of more sophisticated systems. If similar maps could be obtained with a single LA electrode, then perhaps preoperative mapping would substantially reduce operative mapping time.

Thirteen of 21 patients with passive RA activation underwent the radial procedure because the pattern of atrial activation could not be completely determined in the intraoperative analysis. Have they really proved their point?

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Reply to the Editor:

I appreciate the comments by Dr Shanmugam.

All surgical procedures are based on the anatomic or physiologic conditions, and thus so should be the operation for atrial fibrillation (AF). The most challenging but exciting issue in surgical intervention for arrhythmias is that the lesions responsible for maintaining the arrhythmias are not usually visible by use of the ordinary approaches. We found intraoperative mapping to be useful in determining the mechanism of AF and the optimal procedure in each patient. The patients who underwent the simple procedure were selected only on the basis of the results of the analysis in the study. If the patients with right atrial reentry had undergone the simple procedure, the AF would not have been converted to sinus rhythm.

The mapping technique requires further refinement. The pattern of atrial activation could not be determined on the basis of the intraoperative analysis in some patients. In about half of those patients, the atrial activation was complex, and it took several hours to determine the activation pattern through a postoperative offline analysis. Some preoperative examination method should be developed to determine whether the AF is maintained by rapid activation arising from a pulmonary vein (PV) alone or a combination with reentrant physiology and to determine the optimal procedure preoperatively.

A mapping technique would be helpful rather in verifying conduction block across each ablation lesion and in assessing the inducibility of AF. This assessment would be practical and crucial in off-pump AF ablation. The intraoperative mapping would enable a stepwise approach to AF in the off-pump setting, in which the atria could be mapped again after the successful isolation of the PVs to determine the indication for atrial linear ablation and the location of each lesion.

The transition from intermittent to continuous AF might be contiguous and related to the pathophysiology of the atrial myocardium, such as fibrosis of the myocardium combined with spatiotemporal dispersion of the refractoriness and conduction velocity. These abnormalities might be correlated to the left atrial (LA) diameter, the duration of AF burden, and other clinical conditions. There was an insignificant difference in the LA diameter between the patients with continuous and intermittent AF in our clinical experience. However, we believe the procedure should not be determined on the basis of the clinical findings alone because all those clinical parameters did not directly correlate to the above pathophysiologic conditions.

Most of the patients exhibited coexisting repetitive activations arising from 2 or more PVs. Interestingly, the distribution of the focal activation was dominantly from the right and left superior PVs. Isolating the right or left superior PV alone with an LA cuff would be technically difficult and unsafe. The superior and inferior PVs should be isolated bilaterally from the respect of prophylaxis and the surgical technique.

The focal activation from the LA appendage might be an epicardial breakthrough of the activation arising from the left superior PV, conducting through the pectinate muscle endocardially. The LA appendage should be excised not because there can be a focal firing from the appendage but because the risk of thromboembolism should be reduced by all possible means in the patients who are not cured of AF or whose LA contraction was not sufficient after the operation.

Intraoperative mapping of AF is not as simple as we have experienced in Wolff-Parkinson-White syndrome. The results of the data analysis might not necessarily be useful in each patient but might be helpful in understanding this troublesome tachyarrhythmia and in developing a definitive procedure.

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Performance of surgery for congenital heart disease: Shall we wait a generation or look for different statistics?

To the Editor:

We read with great interest the reviews on surgical performance in the December

2004 issue of the *Journal*.[1-4] These publications made clear that any system that intends to measure surgical performance should include a reliable case-mix/risk-adjustment method. New sophisticated statistical algorithms using risk adjustments are flourishing, such as cumulative risk-adjusted mortality (CRAM), risk-adjusted sequential probability ratio test (SPRT), risk-adjusted cumulative sum (CUSUM), and Funnel plots. A statistical model to adjust for case mix is easier to develop in acquired cardiac surgery than in congenital heart surgery (CHS), which deals with 200 diagnoses and 150 procedures[5] with potentially several thousands variations[6] and a volume that is a tenth of that seen with acquired cardiac surgery.

The impossibility for a pediatric heart surgeon to access a "scientific case-mix yardstick" today is quite frustrating. This problem, specific to CHS, is not addressed in these reviews, when at the same time the epicenter of the wave that affected the British cardiothoracic surgery community started at the Bristol Royal Infirmary, precisely in a pediatric cardiac surgery unit. It is reasonable to believe that the effect of the Bristol affair would have been considerably lessened if a congenital case-mix evaluation method had been available.

In light of the tremendous statistical challenge in building objective outcomes data for CHS evaluation, 2 new approaches were recently developed. Jenkins and colleagues[7] have developed a consensus-based risk-adjustment scheme for CHS named RACHS-1. Lacour-Gayet and associates[8] have developed the Aristotle score, a method of complexity adjustment to evaluate surgical results with input from 50 surgical centers from 25 countries. These 2 projects are based on subjective probability and rely on expert opinions.

As surgeons, cardiologists, and morphologists dealing with a challenging specialty, we believe that at present, no reasonable risk-adjustment system exists. The conclusion given by Eugene Blackstone[1] in the *Journal's* December 2004 issue was as follows: "Unfortunately, risk adjustment tends to be particularly incomplete when there are rare or multiple measured, unmeasured, or unevaluated risk factors present, so the search for adequate unconfounded quality measures should go on." The crucial question is this: How long can we wait when the insurance companies, the media, and the courts spend their time measuring quality on the basis of totally subjective and potentially false outcome data? Many congenital cardiac surgeons feel responsible for creating a fair and reliable method to evaluate surgical performance applied to centers and individual surgeons.

The Aristotle project, which is necessarily based initially on subjective probability, is under statistical evaluation. We believe that we cannot wait a generation or longer to "let the data speak for themselves"[9] and be finally protected by a fair and reliable evaluation of quality. Our hypothesis is that the surgical-based knowledge involved in the Aristotle project will improve the test characteristics of the statistical models. Whether using subjective probability information combined with objective data will produce better statistical models than using objective information alone is a question that is testable. Our hope is that by combining the surgical-based knowledge obtained from the Aristotle project with objective data and appropriate modern, rigorous statistical theories, we can create a better adjustment tool to assess quality and performance in CHS.

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